X. "Experiments on the Discharge of Electricity through Gases. (Second Paper.)" By ARTHUR SCHUSTER, F.R.S., Professor of Applied Mathematics in Owens College. Received May 18, 1887.

Three years ago I gave a sketch of a theory of the passage of electricity through gases, in which it was assumed that in the gaseous discharge an atom carries a definite molecular charge just as in electrolysis (see Bakerian Lecture, 'Roy. Soc. Proc.,' vol. 37, p. 317). I showed how this molecular charge if constant might be measured, and I have since that time worked continuously in arranging for the necessary experiments.

I have, with the help of a grant from the Royal Society, mounted a battery which gives me an electromotive force of 1800 volts, which I hope will be sufficient for my purpose. The experiments which I have in view, involve the accurate measurement of the electric potential at different points of a vessel through which a discharge is passing; but before these could be undertaken a number of intermediate questions had to be settled by experiment. As these have in this way established some definite points which I believe to be of importance, I venture to bring them before the Society.

In thinking over the phenomena presented to us in vacuum tubes, I always felt a difficulty owing to our ignorance of the conditions which hold at the surface of bodies, either suspended in or near the discharge, or even at the boundary of the vessel through which the discharge is passing. It is evident enough, that if there is a flow of electricity on the surface of a non-conductor that flow must be tangential, but it is not so clear whether we are justified in concluding from this that there can be no normal forces at such surfaces, for it is not necessary that the flow should always take place along the lines of force. Imagine, for instance, a discharge to consist of particles charging at one pole, then moving, under the action of electric forces, to the other pole; and let the vacuum be sufficiently good that few or no encounters take place while it passes from one pole to another. Then bring an electrified body near the discharge. That body may deflect the particles conveying it, but unless its electrification is sufficiently large, it will not draw up the discharge to its surface, so that the normal forces on the electrified body which has been introduced need not be neutralised by the discharge.

The question is one altogether for experiment to decide. Supposing we suspend two pieces of gold leaf, as in an electroscope, at any place in a partially exhausted vessel, and render them divergent by electrification, they should collapse as soon as the discharge begins to pass, if tangential forces only can permanently exist at their surface. This I have tested by experiment and found to be the case.

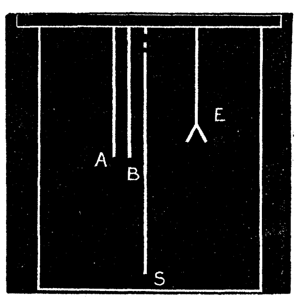
A cylindrical glass vessel, 38 cm. high and 15 cm. wide, was divided into two approximately equal compartments by a vertical metallic screen. There was an open space of about 5 mm. between the screen and the sides of the vessel, a space about 4 cm. above, and 2·5 cm. below the screen. One compartment contained two pieces of gold leaf, which could be charged from the outside. The other compartment contained two electrodes about 5 cm. apart, and 2 cm. from the screen; these distances could be varied during the experiment. The screen was always conducted to earth, and the electric fields on the two sides of the screen were therefore nearly independent of each other. When the gold leaves were electrified and divergent, and discharges from the induction coil passed between the electrodes on the other side, no effect could be observed at atmospheric pressure: the gold leaves remained divergent.

At a pressure of about 4·3 cm. of mercury, the effect I was looking for first appeared; when the discharge passed, the divergent leaves slowly collapsed, and as the pressure was further diminished the collapse took place more and more quickly.

We have here then even with the discontinuous discharge a neutralisation of all normal forces at the surface of the gold leaf.

Fig. 1 illustrates in a diagrammatic form the arrangement of the experiment, A and B being the electrodes, S the screen conducted to earth, and E the gold leaf.

FIG. 1.



In previous experiments, which were not so satisfactory from another point of view, and which I therefore do not quote, the same effects were observed both with the continuous currents from the battery and the discharges from the coil; the only difference being, that the effects due to the continuous discharge were more rapid and regular than those due to the induction coil. As soon as the pressure was sufficiently reduced to allow the continuous discharge to pass, the destruction of normal forces took place.

It seemed to me to be interesting to observe more particularly the

effects of the ordinary discharges we have at our command, at atmospheric pressure. I took two light balls, and suspended them so that they could be made to diverge by electrification. The electrodes (either spheres or points) of a Voss machine were placed at a distance of 3 inches from each other, and the electrified balls were placed at a distance of 9 inches from the discharge. The results are contained in the following table, in which the two first columns indicate whether the electrodes of the Voss machine were points or spheres. The third column gives the electrification of the balls, and the fourth column the results.

Negative electrode.	Positive electrode.	Balls.	Result.
Sphere	Sphere	Positive	Balls collapse slowly.
"	"	Negative	" remain divergent.
Point	Point	Positive	" collapse quickly.
"	"	Negative	" remain divergent.
Sphere	"	Positive	" " "
"	"	Negative	" collapse slowly.
Point	Sphere	Positive	" " quickly.
"	"	Negative	" remain divergent.

It will be seen that when the two electrodes are similar, whether spheres or points, the balls collapse when they are electrified positively only; but that when one electrode is a sphere and another a point, the balls collapse if their electrification is of the opposite nature to that supplied by the point.

I shall refer to the theoretical bearing of these experiments at the end of the present paper, but wish at once to point out, that the apparent difference in the results, for positively and negatively electrified balls, can be one of degree only, and not one of kind. If the balls are positively electrified, they collapse when the two electrodes are similar; but in the other case, when the balls are negatively electrified, an equal and opposite charge will be found on the objects placed in the room, or on the walls. If this positive electricity is neutralised by the discharge, the balls must ultimately collapse in this case, as well as when they were originally positively electrified. To test this argument, I placed the balls in a glass case partially covered on the inside with tinfoil. The inside of the case was nearly a cubical space of sides 37 cm. long. The front of the case was taken out, and the discharge was taken between the two points near the open front. The balls now collapsed, whatever their charge was to begin with, but more quickly when they were positive than when they were negative.

Confining ourselves to the case of a discharge between points and

positive balls in its neighbourhood, the question remains whether they will collapse ultimately, whatever their distance from the discharge, or whether there is a finite distance beyond which no effect can be observed, even if the discharge be continued indefinitely. Without wishing to express any final opinion on the point, I may yet give the impression I have obtained from the experiments I have made. I believe that, whenever we have a continuous and steady discharge in an inclosure, however large, the complete neutralisation of all normal forces on surfaces through which no current goes is only a question of time, but that if there is any discontinuity in the currents (as I believe is the case in all discharges at atmospheric pressure) there is a definite distance (depending on the time intervening between two discharges) beyond which no effect will be observed.

The conclusion thus arrived at, which will be proved beyond possibility of doubt in the second part of this paper, is this: *we can only have tangential forces at the surfaces of vessels enclosing a gas through which a discharge is passing*, provided no current crosses the surface. It may be that this conclusion will appear evident to some without experimental proof, but I found it necessary to obtain definite evidence, because the fact itself has been constantly neglected and disregarded.

Thus, for instance, it is found that electrified bodies placed outside a vessel through which a gaseous discharge is passing, do not permanently affect the appearance of the discharge, and this fact is commonly taken to prove that there can be no free electricity of either kind in the discharge. But it follows from the surface condition at the inside of the vessel, that this surface must act as a complete screen between the electrified bodies placed inside and those placed outside the vessel; and the experiment therefore proves nothing.

In similar fashion, Goldstein observed that certain actions of one negative electrode on another were destroyed when a screen was interposed between them. The results obtained in this paper give the obvious explanation of this fact.

After I had convinced myself that an electrified body placed in a partial vacuum through which an electric current is going, has its electricity quickly neutralised, it was doubtful still whether this neutralisation was due to an actual discharge or merely to a covering of electrified particles of an opposite sign. The question is a vital one in all cases where potentials have to be measured. For we can only measure potentials of a gas by measuring the potential of a metal in contact with it; and if an electrified body is covered by electrified particles of a different sign, there is a finite difference of potential between the metal and the gas, and we should have to inquire carefully, in each particular case, how far such a difference would affect our conclusions.

Hittorf has measured with great success the fall of potential at different points of a vacuum tube through which a discharge was passing, and none of his principal conclusions are affected by these scruples, for he gives in his paper sufficient evidence that his method is applicable to the cases he has examined. But the purposes I have in view rendered a measurement of potential necessary under severe conditions, in which a serious error might have been introduced by assuming without verification that the potential of a metal is the same as that of a gas in contact.

Strictly speaking, the potential is always continuous as long as we are dealing with finite charges, but when a layer the thickness of which extends to molecular distances has its sides charged with opposite electricity, it is customary to compare the two sides of such a layer directly with each other, and neglecting the rapid variation of potential within the layer, to speak of a discontinuity of potential. It is in this sense that I am here speaking of a possible finite difference of potential between a metal and the gas in contact.

The question is settled by the principal result of this paper:

A steady current of electricity can be obtained in air from electrodes at the ordinary temperature which are at a difference of potential of one quarter of a volt only (and probably less); provided that an independent current is maintained in the same closed vessel.

In other words, a continuous discharge throws the whole vessel into such a state that it will conduct for electromotive forces which I believe to be indefinitely small, but which the sensitiveness of the galvanometer I used has prevented me from tracing with certainty below a quarter of a volt. There cannot be therefore a finite difference of potential between a gas and a metal in contact greater than that amount.

Hittorf,* who has done more to clear up this subject than anyone else, has found already that a current from a few cells will pass crossways through a discharge in vacuo, but his auxiliary electrodes were introduced into the discharge itself, and it was doubtful, therefore, how far the results were due to the high temperature of the particles carrying the luminous discharge.† I was aiming, on the contrary, at placing the secondary electrodes as far from the main discharge as possible, and at rendering by means of screens the two electric fields as independent of each other as possible.

I need not describe all the successive experiments in which I have endeavoured to make my tests more and more severe. It will be

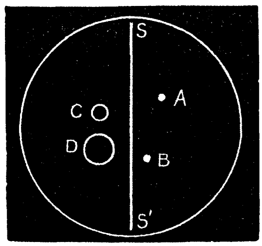
* 'Wiedemann, Annalen,' vol. 7, p. 614.

† That high temperature itself is a matter on which authorities differ. I do not desire at present to commit myself to any view which seems to me to involve a definition of "temperatures" under exceptional circumstances.

sufficient to give an account of those experiments which I consider most conclusive.

The same vessel was used as in the previous experiment; the screen was not so long, but left a space of 10 cm. free at the bottom. Fig. 2 will explain the arrangement. SS′ is the screen, always conducted to

FIG. 2.



earth; A and B are the electrodes for the main discharge; C and D are the auxiliary electrodes, which were of the form of copper cylinders, 4 cm. high, and having a diameter of 1 and 1·8 cm. respectively. Their distance was 2 cm., and their axis was 2 cm. away from the screen. Of the main electrodes, one was 2 cm. and the other 1 cm. away from the screen; their distance was about 4 cm. The discharge from the battery was always used. Whenever a steady current passed between A and B, an auxiliary battery of Clark cells would send a steady current between C and D, and the lower limit of electromotive force capable of producing a measurable current seemed only to depend on the delicacy of the galvanometer.

Thus, for instance, at a pressure of about $\frac{3}{4}$ mm., a current of 0·11 ampère was sent from A to B; and the following currents were obtained when the poles of an auxiliary battery were connected with C and D, the galvanometer being inserted in the auxiliary circuit:—

40 Clark cells	....	0·032 micro-ampère.*
20 ,,	....	0·021 ,,
10 ,,	....	0·014 ,,
1 Leclanché	....	0·005 ,,

On another day I tried to reduce the electromotive forces still further. The galvanometer, however, had for this purpose to be rendered so astatic that the always changing torsion of the fibre suspending the mirror was a source of serious trouble; for this reason the numbers have not much value in themselves, but there was no

* 1 micro-ampère = 1 ampère × 10^{-6}.

doubt in each case as to the existence of a current. The whole effect was smaller on that day for reasons which could be traced.

1 Leclanché	gave	a current of	0·0010	micro-ampère.
5/18	,,	,,	0·0011	,,
1/6	,,	,,	0·0003	,,

The main current in these last experiments was 0·008 ampère.

An electromotive force of one-sixth Leclanché is about one-quarter of a volt, and a current has thus been obtained in a gas from an electromotive force which could not maintain a current through water.

An electromotive force of 0·1 volt gave doubtful results, but this was probably due to the experimental difficulty of detecting the current.

In some previous experiments, which, however, were not quite free from objection on other grounds, the lowest electromotive force for which the currents could be measured was 0·2 volt.

The experimental arrangement which is the best for the qualitative investigation of the effect is not the best for quantitative measurements, and I have therefore not endeavoured to follow out to any great extent the quantitative laws of these currents produced by low electromotive forces. I may give, however, some facts which I have observed. The intensity of the current depends on a great many circumstances.

1. It increases rapidly with the intensity of the main discharge, and also with a reduction of pressure, as far as I have tried it (that is about $\frac{1}{2}$ mm.).

2. The intensity of the current from the auxiliary battery increases less rapidly than the electromotive force.

3. In some experiments in which one of the electrodes of the auxiliary battery was a copper wire and the other a copper cylinder, the current was nearly always considerably stronger when the larger surface was the kathode.

4. Anything that facilitates the diffusion of gas from the main current to the auxiliary electrodes will increase the strength of the current observed. In some experiments, in which the screen separating the two fields was made of wire gauze instead of tinfoil, the currents were stronger than those given above.

5. In the arrangement shown in fig. 2 the currents were stronger when the main electrode A was negative than when it was positive.

Considerable care has to be taken, especially when no screen is used or when it is not conducted to earth, in order to avoid leakage currents. However well the battery may originally have been insulated, the insulation always grows worse with time (owing to dust and moisture). If, then, any part of the auxiliary circuit itself is not pro-

perly insulated, we easily get a current through the galvanometer which is nothing but a branch current from the main discharge. Such a leakage current, even when it is weak, considerably increases the effects described in this paper.

These experiments show conclusively that there is nothing peculiar in the gaseous state of a body to prevent any electromotive force however small from producing a current. If a finite electromotive force is required under ordinary circumstances the fact cannot be accounted for, as Edlund and others have attempted to do, by a special surface resistance which has to be overcome by a finite difference of potential at the surface.

I think the facts are very well accounted for by the theory which I have proposed in my last paper. If the two atoms of a gas making up the molecule are charged by opposite electricities, but are held together in addition by molecular forces, a finite force is required to overcome the latter. But as soon as that force is overcome and the atoms themselves are set free to diffuse and constitute a current, these atoms will be able to follow any electromotive force which we may apply. If, then, we have auxiliary electrodes, these electrodes will establish their electric field which we can never screen off completely from any other part of the vessel except by closed surfaces. The atoms, with their positive and negative charges, will diffuse across to the auxiliary electrodes and give off their electricity to them. No finite difference of potential is required in the auxiliary electrodes, because even if there is work done in making an atom interchange its positive for negative electricity, that work is undone again at the other pole, where atoms of a similar kind interchange negative for positive electricity.

This I believe to be the general explanation of the phenomena described in this paper. In order to account for the peculiar difference between positive and negative electricity which appears in the experiments done at atmospheric pressure, also those mentioned under 5 (p. 377), we must make some further supposition. I have already mentioned in my last paper that, according to the theory I have proposed, we must imagine the molecules to be broken up at the negative pole, and I believe that this fact will ultimately be found to account for this apparently unsymmetrical property of the two electricities; but I should like to strengthen my case by further experiment before going into details on this point.

I should like, in conclusion, to point out an important application of these results. I have last year obtained by calculation results which seem to show that the principal cause of the diurnal variation of terrestrial magnetism is to be looked for in the upper regions of the atmosphere. Professor Balfour Stewart at various times suggested that the air currents in these regions may, owing to the lines of

force of terrestrial magnetism, have electric currents circulating in them.

The difficulty against this supposition always seemed to me to lie in the fact that the electromotive forces required to start a current were larger than those which could possibly exist in the atmosphere. But as there are very likely continuous electric disturbances going on, such as we observe in auroræ and thunderstorms, the regions within which these discharges take place would act as conductors for any additional electromotive force however small, so that any regular motion, such as tidal motions, could very well produce periodic effects affecting our magnetic needles.

If these original discharges increase in importance, then, according to the results obtained in this paper, the currents due to the smaller periodic causes would increase also, and they may increase in a very rapid ratio. We know that the electric discharges in the upper regions of the atmosphere are considerably stronger at times of many sunspots, and this may account for the fact that at those times the amplitude of the daily oscillation of the magnetic needle is considerably increased.

I have had considerable assistance in these experiments from my assistant, Mr. Stanton, to whom my best thanks are due.

XI. "Contributions to our Knowledge of Antimony Pentachloride." By RICHARD ANSCHÜTZ and P. NORMAN EVANS. Communicated by Prof. A. W. WILLIAMSON, For. Sec. R.S. Received May 5, 1887.

Some months ago* we showed that antimony pentachloride can be distilled, undecomposed, under much diminished pressure; our next step was the attempt to determine the vapour-density under similar conditions. The fact that the boiling point of antimony pentachloride lies much lower than that of the trichloride would seem to show that the vapour-density of the pentachloride, as in the case of the trichloride, corresponds to the simpler formula. Nevertheless, on account of the fundamental importance which the establishment of the simple formula $SbCl_5$ would have for the valence of antimony, it seemed indispensably necessary to make a determination of the vapour-density. We will preface our further observations with the remark that we have not yet succeeded in determining the vapour-density of antimony pentachloride under diminished pressure; however, in the course of many unsuccessful attempts which we have made to this end, we had one point thrust on our notice, which on

* 'Chem. Soc. Journ.,' vol. 49, 1886, p. 708.

FIG. 1.

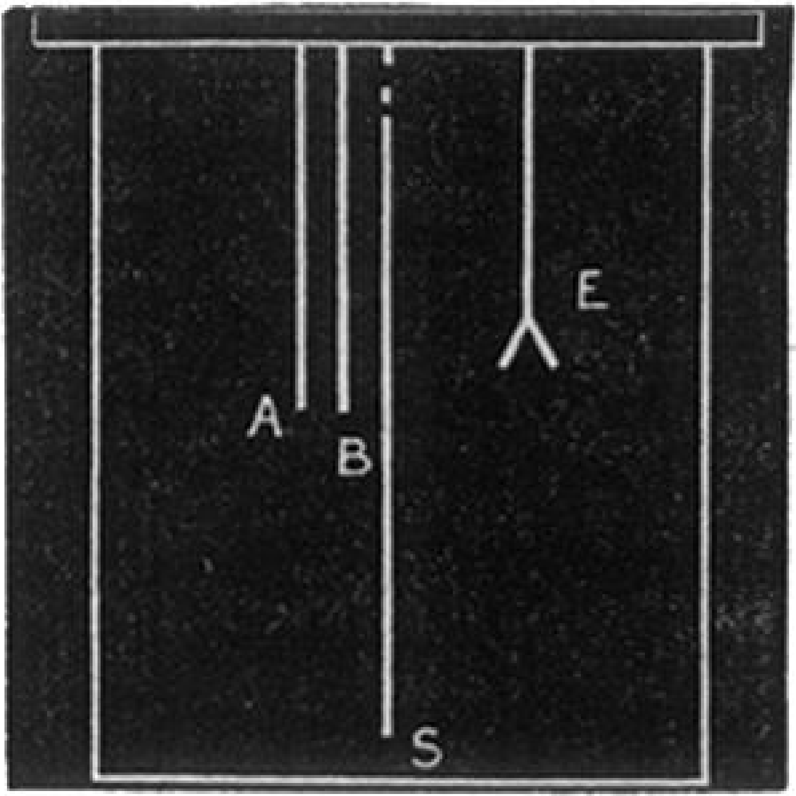


FIG. 2.